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A N
INAUGURAL DISSERTATION,
TO DISPROVE THE
EXISTENCE OF MUSCULAR FIBRES
IN THE VESSELS.

SUBMITTED TO THE PUBLIC EXAMINATION
OF THE
FACULTY OF PHYSIC,
UNDER THE AUTHORITY OF THE
TRUSTEES OF COLUMBIA COLLEGE
IN THE
STATE OF NEW-YORK:
WILLIAM SAMUEL JOHNSON, LL.D. President;
FOR THE DEGREE OF
DOCTOR OF PHYSIC;
ON THE THIRTIETH DAY OF APRIL, 1793.

By JOTHAM POST, A. B.
Citizen of the State of New-York.

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Samuel L. Mitchill.

T O

RICHARD BAYLEY,

Professor of Anatomy,

A N D

WRIGHT POST,

Professor of Surgery,

IN COLUMBIA COLLEGE:

I N

Grateful Testimony for their Care and Attention to him, in his
Medical Studies,

T H I S

D I S S E R T A T I O N

I S A D D R E S S E D,

By their Pupil,

The AUTHOR.

Doctor John Onderdoonk
From his friend, and
Well wishes
The author

INAUGURAL DISSERTATION, &c.

SINCE the discovery of the circulation of the blood, the attention of Physiologists has been excited to ascertain the manner in which it is carried on. The heart is universally allowed to be the principal moving power; but many supposing it incapable, of itself, of propelling the blood through the system, have conjectured means, by which its action might be assisted, and the routine of the circulation facilitated. They have supposed the vessels of the sanguiferous system to be encircled by muscular fibres, which have an alternate contraction and dilatation, by which an additional power is obtained for keeping up and carrying on the circulation.

An examination of this subject is of more importance than might at first be imagined; for upon a supposition of the existence of muscular fibres in the arteries, has been founded an opinion, which is now becoming very general, of the proximate cause of

many important diseases.* It is true, such an opinion, at first view, exhibits some degree of plausibility; but upon more accurate investigation, it appears to be rather the creature of imagination, than the result of impartial observation and deliberate reasoning.

Many eminent characters have made experiments to ascertain the projectile force of the heart†, some of which have made it almost inconceivably great, while others have confined it to a smaller compass. But however they may differ, it is still proved beyond a doubt, that its strength‡ is considerable, and to those who are willing to be unbiassed by prepossessed opinions, sufficient to propel the blood through every minute ramification of the arteries, and veins, independent of their muscular power, admitting such. Why then should we fly to the aid of a cause, the assistance of which is not at all requisite?

The opinion, that muscular fibres encircling the vessels are necessary for the circulation, most probably must have originated from conjecture. But
conjecture,

* Among these, fevers and inflammations hold no inconsiderable rank. See Cullen's First Lines, volume first, subject pyrexia. See Bell and Cullen on the proximate cause of inflammation.

† See Hales's Hæmæstaticks. See Keil's Anatomy, and Calculations on the Force of the Heart, &c.

‡ By the words STRENGTH and POWER, which are used throughout this Treatise, I would not wish to be understood to mean any great exertion. The terms are used for want of more expressive ones.

conjecture, though sometimes of use to the ingenious in the discovery of truth, unless well supported by fact, should never be mistaken for demonstration.

The learned Haller,* who has applied much care, and attention to the investigation of physiological subjects, supposed the existence of muscular fibres in the vessels as requisite to assist in carrying on the circulation, yet expressly declares that he never could detect any muscular fibres, nor perceive any contraction, in the arteries, though he had used microscopic instruments for that purpose. This is a singular instance of prejudice, to assert the essentiality, and believe the existence of muscular fibres in the vessels, as requisite for carrying on the circulation, though unsupported, and in a manner contradicted by his own experiments. But what may we not expect from researches made to support a favourite hypothesis!

If, from the presence of muscular fibres on the aorta† near its origin from the heart, we infer their existence throughout the system of circulation, we may commit an error; for whenever we meet with them there, which, in fact, is but a rare occurrence, they

* Vide experimenta de irritabilitate, sect. 2d.

† Because in some instances they have been seen on the aorta, the existence of muscular fibres by analogy has been inferred throughout vessels of the circulating system.

they are most probably the consequence of some morbid affection. I am confirmed in this opinion from the following circumstance. There is in the Anatomical Museum of Columbia College, a preparation of an aorta, with an incurvated spine. The aorta, just below its curvature, has become so much enlarged as to form a complete aneurismal sac. Round this sac, and the parts adjacent to it, there was a number of muscular fibres plainly visible to the naked eye. But instead of forming the middle coat of the aorta, as is generally imagined and asserted, they were external to the common loose cellular coat of the artery, and had very little connection with each other. Neither did those which were on the aorta wholly embrace it. The part most remote from the disease, had not the least appearance of them. From this, and from similar cases, as well as from the fact, that, in subjects really found, these fibres are not to be discovered, we may conclude, that they only exist as the consequence of disease. For no Physiologist will deny, that muscular fibres, or at least what appear to be such, may arise from the morbid affection of membranous parts.

I have repeatedly examined the aortæ of oxen, sheep, and other animals, as well as of human subjects; but have never, while in a sound state, been able to discover the least vestige of muscular fibres in them.

Upon

Upon the supposition of the existence of muscular fibres in the vessels, as essential to the circulation, and an increase of their influence as they recede from the heart, I supposed they might be found in the iliac arteries, but when I examined these, not the smallest appearance could be observed to justify the opinion. However, supposing I was not far enough distant from the heart, I imagined they might become more evident in the tibial and smaller arteries, which could be easily seen; for, according to the advocates for muscular power in the vessels, these are less under the influence of the heart, and of course, the presence and action of this power become more requisite: but here again, upon examination, not the least appearance of muscular fibres could be discovered.

Unwilling, however, to relinquish the idea, from its being so very generally believed, that there was a *systole* and *diastole* in the arteries, because I could not discover the presence of muscular fibres in them with the naked eye, I supposed they might be so small as to elude the sight, and that their contraction and dilatation would become evident by their effects. I accordingly attended to the flowing of blood from the carotid arteries of animals; but could not observe any sensible alteration in the stream as it issued from their mouths, any farther than that it came forth

forth with an increased or diminished velocity, as the heart contracted or dilated. The diameter of the stream apparently remained the same while the vessels were in a state of distention; nor did the sides of the vessels approach each other. This coincides with Doctor Kirkland's* observation, that "so far are the larger arteries from becoming wider or narrower alternately, that their sides have not the least motion of this kind, whether the blood flows freely through them, or whether it is interrupted at the extremity of the divided vessel; which experiment we made to prevent our being mistaken, by any alteration that might take place from the resistance to the blood being entirely removed; but the sides of the vessels were greatly distended, yet they were perfectly still and quiet." Instead, therefore, of concluding that the vessels of the sanguiferous system are encircled with muscular fibres to assist in the circulation, we are induced to adopt a contrary belief.

If the arteries have muscular fibres, and are of course irritable, they would, before life itself becomes extinct, shew some signs of it, when put to the test of experiment. But this, however, did not appear; for, having taken a carotid artery from an ox immediately after his throat was cut, I threw into it some
water,

* See Kirkland's *Medico-Chirurgical Treatise*, volume the first, page 339.

water, about the temperature of the blood, but could not observe the least contraction in it, unless when distended; and then, by means of the elastic membranes of which it was formed, part of the water would pass out. I am informed, that an experiment of this kind has been considered absolutely conclusive of the existence of muscular fibres in the sanguiferous system as essential to the circulation of the blood. The conclusion, however, must have been hastily made; for any person, who has impartially and carefully attended to the experiment, might have been convinced, that the arteries were elastic tubes, and of consequence, if put upon the stretch, must necessarily contract, and force out the fluid from within, as if by a muscular contraction; and the velocity of the fluid rushing out would be nearly proportionate to the distention made.

In order farther to illustrate the truth of the opinion, that the aid of muscular fibres encircling the vessels of the sanguiferous system is in no wise requisite, or essential to the circulation, I laid bare the carotid artery of an ox immediately after his throat was cut, and tied it in two separate places, that between the ligatures the artery might be filled with blood. I then punctured it with a lancet, when the blood, instead of being forced out by a contractile and dilating power, as it must have done if muscular agency

agency had been the cause, regularly trickled down the sides of the vessel. The artery, however, was not much distended; otherwise, from the re-action of the elastic power, I suppose the blood would at first have issued out in a stream.

At the Anatomical Theatre of Columbia College I have seen Professor Bayley lay bare the carotid artery of a dog, nearly three inches in length, in order to ascertain whether it would contract or not. We examined it with close attention, but could not perceive any motion in it, either of contraction or dilatation.* This, too, perfectly agrees with what Haller himself observes; “for upon examining the circulation in animals with a microscope, I have never observed any contraction in the arteries. I have viewed the circulation for whole hours in fishes and frogs, and during the whole time the sides of the vessels remained as quiet as those of the tube with which I examined them.” Doctor Kirkland’s experiments on the same subject coincide with those of Haller.†

So far experiments seem directly conclusive, that the arteries and veins, excepting their elasticity, are passive conductors to their contained fluids. Observation

* The pulse, however, was plainly felt at every contraction of the heart.

† See Kirkland’s *Medico-Chirurgical Enquiry*, volume first, page 310.

vation and reason may afford us many more arguments to confirm the opinion.

From the structure of the heart, we may draw very forcible reasons, to induce us to believe, that the arteries and veins afford no other assistance in the circulation, than that which arises from their elasticity. The right ventricle, compared with the left, has but very little strength. It has not to send the blood so great a distance, nor through so complicated a course, and therefore so much strength is not necessary. But the left ventricle, which has to propel the blood through the most distant parts of the body, as well as those which are less remote, has its power proportionably greater. Does not this great disproportion in the strength of the ventricles afford us a striking argument in favour of the opinion, that Nature has placed in the heart the separate power of carrying on the circulation?

If the arteries and veins are endowed with a power for carrying forward their own contents, why the necessity of a heart endowed with so much muscular force? or, indeed, why the necessity of a heart at all? Let the blood be but placed in the arteries and veins, and they of themselves would be capacitated to keep up the circulation.

Had the arteries been originally designed to have had a contractile and dilating power from the influence of muscular fibres encircling them, some more convenient method might have been taken to bring them into action. We cannot suppose that Nature, ever regular and simple, would perform her functions by indirect and complicated means in man, her favourite, especially where complication appears entirely unnecessary. But this would evidently appear to have been the case, should we allow the existence of muscular fibres in the vessels; for between the coat which is said to be muscular, and the blood which is to excite it into action, is interposed a firm dense membrane.* Membranes, in a sound state, are insensible:† Therefore, if the arteries have muscular fibres separated by this insensible body from the blood which is to act upon them, they cannot be brought into action by any direct means; and to suppose that Nature ever designed much farther assistance in the circulation, than that which arises from the heart, connected with the elasticity of the vessels, is detracting from her perfections. If farther assistance was requisite, why were not the vessels made into hollow muscles?

Nature

* See Haller's First Lines, section 31. See Falconar's Synopsis, page 16.

† Noted from Professor Bayley's lecture on membranes, and membranous parts.

Nature is invariably the same in her different species of creation; and hence, if muscular fibres have been seen in the vessels of one animal in a natural state, it is presumable that they would be demonstrable in another of the same species, which is contradictory to experience.

On perusing some notes taken from Doctor William Hunter's lectures in 1771, I find that he does not admit of any other assistance from the vessels of the sanguiferous system, excepting their elasticity, than as they serve to conduct the blood to the various places to which it is destined. He supposes the heart to be an agent in the circulation, and that in general it exercises but little of its strength. However, he supposes what he calls *nervous energy* to have much influence, and that the circulation in ordinary cases is almost wholly carried on by it. The authority of so great a man, in discarding the idea of the existence of muscular fibres in the vessels to assist the circulation, should have great weight; for he never would have inculcated such a doctrine, had he not been confirmed in it by actual observation and reasoning.

Doctor Whytt,* who has written so learnedly on the vital and involuntary motions, when speaking of the

* See his Observations on the Vital and Involuntary Motions, subject, systole and diastole of the arteries.

the systole and diastole of the arteries, attributes their contraction principally to their elasticity. He appears to have adopted the notion of the presence of muscular fibres in them, only because it had been so generally believed,* without really enquiring into the merits or demerits of the hypothesis.

Mr. Winflow, when speaking of the circulation of the blood through the liver, appears to have adopted the notion of the passage of the blood through the vessels without the aid of muscular fibres; for he observes, that those “three kinds of venal blood† meet in the trunk of the vena portæ ventralis, where they are mixed together, and from thence they enter the transverse sinus or trunk of the vena portæ hepatica. In this sinus they are still more intimately mixed, as in a kind of lake, and become one uniform mass of blood, which being forced into the branches of the vena portæ hepatica only by the supervening blood from the other vena portæ, and by the lateral pulsations of the ramifications of the hepatic artery, its course must be very slow.”‡

The

* This is often the case, that because an opinion is generally prevalent, we readily subscribe to it, whether right or wrong.

† The blood from the spleen, pancreas, and intestines.

‡ The slowness of the circulation here, however more probably, arises from the greater capacity of the veins compared to the arteries, which carry blood to the abdominal viscera.

The manner in which the blood passes from the ventricles, and is received by the auricles, will also favour the idea, that the heart is the simple engine for carrying on the circulation. Both ventricles contract, and both auricles dilate at the same instant;* and so much blood as is sent from the former, is received by the latter. The reason of this may appear evident, when we consider that the arteries and veins are in direct continuation, and considered as one continued tube, filled with a fluid which keeps them in a state of distention; and therefore, whatever cause should propel this fluid from the heart in the course of the arteries, will of necessity cause so much as is propelled to pass out of the veins.† Now, the ventricles propel; and as much as they propel, so much the auricles receive. This process must continue, independent of muscular assistance from fibres surrounding the vessels to aid the circulation, as long as the heart is capable of continuing its action.‡

Since,

* *Utriusque AURICULÆ contractio eodem tempore perficitur, relaxato tunc utroque VENTRICULO, et hi rursus eodem tempore se contrahunt auriculis simul relaxatis. Vide Conspect. Medicin. Theoret. auctore Jacobo Gregory, section 419.*

† Hence we may see, why in aneurisms there is an interrupted pulse.

‡ We may hence see why, after blood has been drawn from the body, when there has been a great plethora, the pulse becomes so much more full, free, and easy, as the heart has a better opportunity of performing its office.

Since, according to the muscular hypothesis, some other power is wanting besides the heart; and since an increase of that power becomes more requisite in those parts which are more distant from the center of the circulation, the arteries, which are called capillary, should be furnished with muscular fibres, which, did they exist, would be easily demonstrable, and the veins, from an external view, would represent nothing but mere fleshy cylinders. This, however, is contradicted by experience; for the coats of the capillary, or extreme arteries, are so thin and delicate as to become perfectly transparent, and the veins, to appearance, are entirely destitute of the least vestige of muscular fibres; neither has demonstration ever proved the existence of muscular fibres in them.

If we admit the existence of muscular fibres in the sanguiferous system as requisite for the circulation, it cannot be readily conceived how the blood retains its progressive motion; nor can it be conceived how muscular fibres produce this effect: for if they are “actuated to their office by a stimulus, they are incapable of being suddenly lengthened by it, and being irritated, they instantaneously shorten themselves, and spontaneously return again to their natural state, when the irritating cause ceases to act.”*

They

* See Kirkland's Medico-Chirurgical Enquiry, subject, inflammation in general, page 260.

They are then as likely to repel, as to propel the blood. This most probably would be the case, if the fibres contracted throughout the system at the same time; for at every attempt made by the heart to send forward the blood, the resistance would be proportionably increased, because the vessels would become more full, and of course increase the action of their muscular fibres by its stimulus; and so, instead of allowing the vessels to dilate, and the blood to be transmitted with greater ease, they would lessen their diameters, and have a tendency to impede the circulation altogether. Neither can we suppose, that, if muscular fibres are present, they perform their office by an undulatory or peristaltic motion, which, in fact, would be somewhat the most plausible supposition, if we are allowed to reason from analogy. Thus the intestinal tube, which has to convey its contents for different purposes, is particularly noticed for its peristaltic motion, which may be easily demonstrated. But it has never yet appeared in the arteries; neither could it exist in them, unless the velocity of its motion was equal to that of the blood itself. But such a supposition is vague and hypothetical. I presume, admitting its existence as a fact, such a motion might be discovered by taking an artery between the thumb and finger. I endeavoured to ascertain this by the experiment in which Profes-

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for Bayley laid bare the carotid artery of the above mentioned dog, as well as in other instances, but did not succeed.

If muscular fibres do really exist in the arteries to assist in the circulation, their contraction and dilatation would cease when the nervous influence, which capacitated them for action, is taken away. It will be difficult then to conceive how the circulation is continued in paralytic cases, particularly in the more general affections; for in these, nervous energy is destroyed almost throughout the body, and muscular motion of course interrupted. It may be asked, then, why the heart continues its action? why does it not partake of the general morbid affection? The reason is evident; because most of its nerves arise directly from the brain; and therefore, while the brain is in any degree capable of performing its function, the heart will continue its action.* Hence we may account for the almost total temporary suspension of the vital powers in syncope.†

There

* This vis insita of Haller may also have an aiding power.

† I have been favoured with the following case, which will hereafter be published more at large, and will have considerable effect in confuting the idea of the presence and action of muscular fibres to aid the circulation. There is a man now living in this city, who fell head-foremost from a load of hay, by which the weight of his body was lost on the basis of the skull. The spinal marrow was concussed; in consequence of which a very general paralysis followed, attended with a disordered state of several of the functions. But notwithstanding this general affection lasted for several weeks, the circulation of the blood continued undisturbed. The

There is a wide difference between the structure of the heart and that of the blood-vessels; and what brings the former into action cannot have the same effect upon the latter. That the properties of the heart and arteries are different from each other, both before and after death, must be plainly visible to any one who has attended to their appearances; for the heart, like other muscles of the body, when life becomes extinct, remains soft and flaccid, while the arteries continue to retain their elasticity. They yield on the application of a distending cause, and resume their former state upon its removal. That the arteries also differ from the heart in a living state must be evident, because they appear not to be affected by the same causes which affect other muscles of the body. What foundation then can there be in the supposition, that the heart and arteries, so discordant in their nature, were destined to perform similar offices?

Some suppose the coats of the arteries to be muscular, because of their strength and thickness. This, however, most probably arises from the continued pulsation of the blood against their sides, because, in

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infancy

pulse was as free and easy as before the injury; and blood drawn from a vein, flowed with its usual celerity. In what manner could the circulation in this case have been aided by the action of muscular fibres, which receive their influence from the nerves? The nerves were incapacitated for action,—Innumerable instances of this kind could be given.

infancy the coats of the arterial and venal systems are nearly of an equal thickness. But if muscular fibres in the vessels are really essential to the circulation, the veins require them more than the arteries, since they are less under the influence of the heart; and therefore they should have been thicker, which is contrary to experience.

When a degree of inanition is produced in the system, the arteries, from their elasticity, will contract considerably, though they do not embrace the blood so strongly as before. The circulation, however, continues, which cannot be attributed to the action of muscular fibres, even admitting their existence in the vessels; for it is difficult to conceive how they possess the property of performing their office equally in one state of the arteries as in another. Yet such is the belief of some who cannot relinquish a former opinion, too deeply rooted by prejudice, for one which is more plain, more characteristic of the simplicity of nature. But it is the disposition of man too often to grasp at things beyond the reach of comprehension, and to neglect those which might be plain and evident to his senses.

We may be farther convinced, that the heart is the organ for carrying on the circulation, from the pulse, which is always affected by the smallest alteration

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tion produced in the action of the heart. Thus, if the pulse be strong, we judge that the action of the heart is strong: If the action of the heart be weak, the pulse is weak; if irregular, it is indicated by the pulse; and should its action cease altogether, the pulse is no more. Further, although the action of the heart continues, should there be a stoppage made to the course of the blood in an artery, the pulse is instantaneously destroyed. Thus, if pressure be made on the inguinal artery sufficient to stop the passage of the blood; at the instant that it is stopped, the pulse below is immediately destroyed, and instantly returns again when the pressure is removed. Now, if there were muscular fibres encircling the vessels, and having a command over them, we cannot rationally suppose that the state of the pulse would so invariably be regulated by the action of the heart. The phenomena which appear in a regular and uninterrupted circulation of the blood, may be imitated by a hydraulic machine. Thus the common pump, for instance, may represent the heart, and tubes leading from it, the arteries; in which may be felt a pulse, when the pump is in action, similar to that produced by the action of the heart itself.

From the fact, that the arteries and veins are mere passive conductors to their contained fluids, abstracting from their elasticity, we can easily explain,

plain, why, when an animal dies from having its throat cut, and life itself becomes extinct, the blood still continues to flow; for, from the elasticity of the vessels, their diameters will be continually lessening as the blood has an opportunity of escaping; and of course, until the arteries acquire their smallest diameters, the blood will be continually and regularly flowing through the given outlet. But if the blood gives a stimulus to the vessels, and they are of course irritable from the muscular fibres surrounding them, this appearance will never take place; for as long as they contain blood, and continue irritable, it will be forced out in a greater or less quantity, as the fibres contract or dilate.

If muscular fibres were absolutely necessary to carry on the circulation, they must of necessity perform their office by alternations of contraction and dilatation. The blood then, of consequence, should pass out of every vessel, both vein and artery, *per saltum*; which is well known to be contradicted by experience: for the very minute branches of arteries, particularly those in the more remote parts of the body, when divided, will not pour out their blood at first *per saltum*, but in one continued stream, as we should *a priori* have supposed, from the idea that the vessels, abstracting from their elasticity, are passive conductors to their contained fluids; but when
the

the parts adjacent to the divided artery become somewhat emptied through the given outlet, the blood rushes out *per saltum*, because they become more full and turgid at every systole of the heart. If there was a contraction and dilatation in the vessels, it should become evident in the veins, as they are so far distant from the influence of the heart; and, of necessity, the blood should pass out in these *per saltum*. But the appearance of the blood issuing from the orifice of a vein in blood-letting, entirely contradicts this opinion.* Indeed, these two facts, that the blood issues from the extreme arteries at first, and from the veins most generally, in a continued stream, might be considered as absolutely conclusive of the *non-existence* of muscular fibres in the system to aid the circulation; for did they exist, the blood must always be forced out with an increased or diminished velocity, according to their contraction or dilatation.

So strongly does common sense confirm us in the belief of elasticity in the arteries, that in common expressions we are forced to mention it to express or explain our ideas. Thus, the celebrated Doctor Cullen, who has taken so much pains to maintain the idea of a spasmodic affection of the extreme arteries

* There may be instances in which the blood will pass out of the veins as if from arteries, as is sometimes the case when there is a very strong, brisk circulation. It also happens if the vein be situated in contact with an artery.

teries in fevers, observes, that their being “ elastic, and constantly endeavouring to contract themselves, they must, on withdrawing the distending force, or, in other words, upon a diminution of the quantity of fluids, be in proportion contracted and diminished in their size: and it may be further observed, that as each part of the vascular system communicates with every other part of it; so every degree of diminution of the quantity of fluid in any one part, must in proportion diminish the bulk of the vascular system, and consequently of the whole body.”* And why shall we complicate the works of Nature, by adding muscular influence, in this instance, to the elasticity of the vessels?

Let us revert to the probable intention of Nature in this instance. Can it be rationally supposed that a multiplicity of co-operating causes would have been brought in to produce an effect, which could have been equally produced by one alone? Would the watch-maker render the machinery of a watch so complicated, could his designed intention be answered by greater simplicity? Instances are not wanting, in which there could not possibly have been any other cause to influence the circulation than the heart alone; for the arterial system has been found almost completely ossified, so that the arteries were no more than

* See his First Lines, vol. iv. sect. 1504.

than bony tubes. Morgagni has noticed this in his celebrated work.* He relates cases which he himself saw, and mentions several authors who had observed the same circumstance.† The arteries, in those cases, could have served no other purpose than that of conducting the blood through the different parts of the body. How then could the patient possibly have lived a day, if the presence of muscular fibres in the arterial system, and their action for carrying on the circulation, had been absolutely essential? There are arteries running through the substance of bones, having their external coats closely adhering to them in their course, and consequently incapable of contractility. In what manner then does the blood circulate through the bones, where there can be no contraction or dilatation? And surely the circulation, in some of the smaller bones, is as remote from the heart, as any of the capillary vessels whatever.

Some, at a loss to account for the circulation of the blood in the extreme arteries and veins, have had recourse to the aid of capillary attraction;‡ others,
to

* See his observations on the arteries in his twentieth letter.

† There is now in the Anatomical Museum of Columbia College, the remains of a preparation, in which the arteries throughout the system were ossified. The person had lived many years, and died in an advanced age.

‡ See Martin's Philosophy, subject, capillary attraction.

to the assistance of the electric aura.* That the circulation cannot be assisted by capillary attraction, is evident; if it was, there never would be a discontinuance of the circulation while the organization of the body remained entire. The capillary influence would always have the same effect; that of keeping up the motion of the blood, which is contradicted by fact. Neither can we rationally suppose the electric aura to be productive of much benefit in the circulation, as the body is but rarely in such a situation as to be positively electrified; and unless it is positively electrified, the electric aura can have no effect in accelerating the passage of the blood through the vessels.

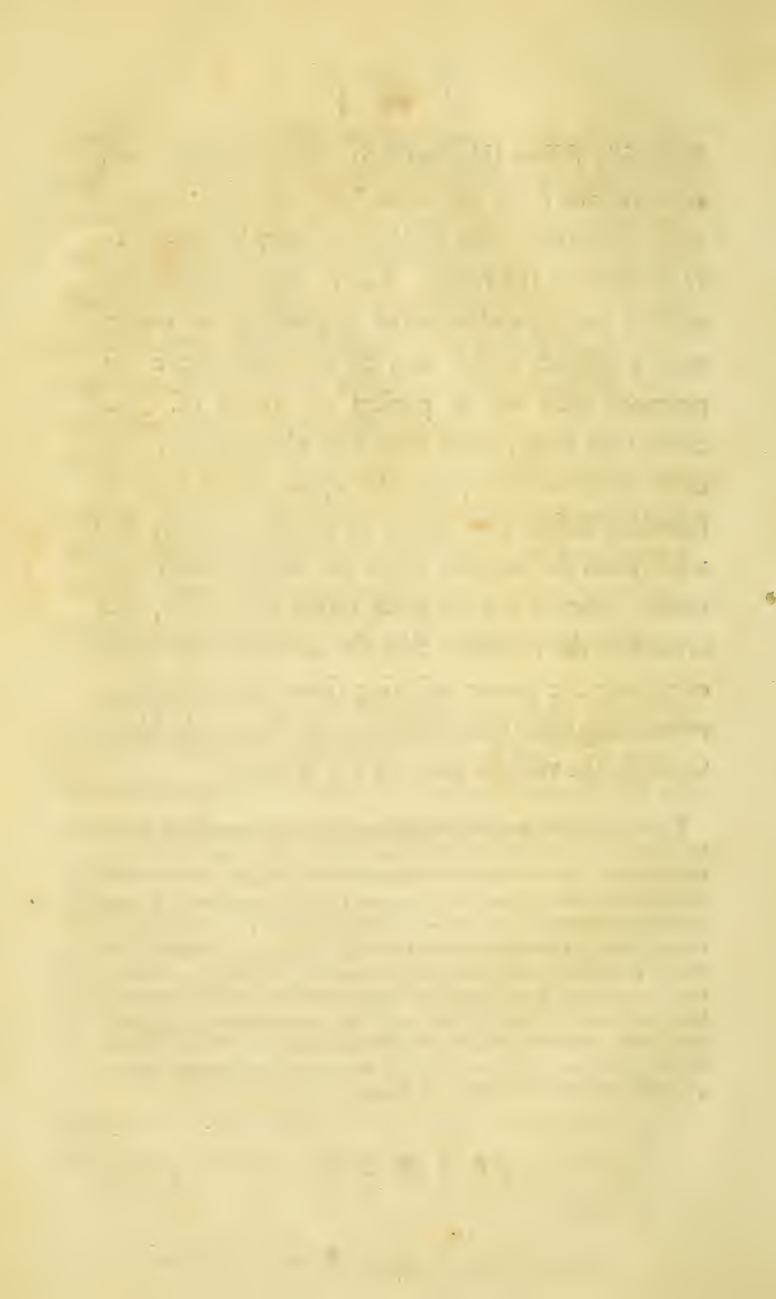
From this review of the subject, which I trust is founded on just observation and impartial reasoning, we may be induced to deny the existence of muscular fibres in the vessels of the sanguiferous system as essential to the circulation. The arguments here offered are founded on simplicity, the principal characteristic of Nature; and when reason and experience confirm the truth, it is improper to be governed by mere assertion or hypothesis, however plausible, or under whatever sanction they may appear. Let us study Nature to ascertain the manner in which her works are carried on; and while we
 implicitly

* See Kirkland's *Medico-Chirurgical Treatise*, page 295.

implicitly follow this method; while we deduce our observations from fact alone, or reasoning, founded upon incontrovertible analogy, we may be more likely to discover the truth. Let no one condemn me, in thus having endeavoured impartially to investigate a subject which may be of much more importance than we at present are aware of. Let those who may peruse these few observations, candidly examine the principles upon which they are founded, before they justify or condemn them; and while calm deliberation takes the lead of hasty conclusion, they will have cause rather to confirm, than invalidate the opinion, that the arteries and veins, excepting the power resulting from their elasticity, are nothing more than passive conductors to the blood through the various parts of the system.*

* I have purposely avoided speaking any thing on the manner in which the chyle and lymph get into the blood, for the want of opportunities to investigate it. But as it appears to me at present, the mouths of the absorbents are to their system, what the heart is to the sanguiferous. I have also avoided attempts to explain any of the phænomena, explicable by the supposition of the increased action of the vessels of a particular part. The bounds prescribed to this Treatise are too small to admit them; and should there be any which at present are not explicable without a belief of muscularity in the vessels, though in these cases they are explained by a supposition, it still does not follow that our principles must fall; for it cannot be supposed that man, who is an imperfect being, can be adequate to the explanation of all the phænomena of Nature.

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